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THE JOURNAL OF PHILOSOPHY

VOL. XIX. NO. 4: FEBRUARY 16, 1922

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PUBLISHED FORTNIGHTLY

THE JOURNAL OF PHILOSOPHY

All communications to the Editors should be addressed to PROFESSOR FREDERICK J. E. WOODBRIDGE, or PROFESSOR WENDELL T. BUSH, Columbia University, New York City.

Subscriptions and advertisements should be sent to THE JOURNAL OF PHILOSOPHY Sub-Station 84, New York City. The annual subscription price (twenty-six numbers) is $4.00 and the charge for single numbers is 20 cents. Advertising rates will be forwarded on application.

The Journal of Philosophy is owned and published by Frederick J. E. Woodbridge and Wendell T. Bush, at Lancaster, Pa.

Entered as second class matter, March 18, 1921, at the Post Office at Lancaster, Pa. under Act of Congress of March 3, 1879.

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VOL. XIX, No. 4. FEBRUARY 16, 1922

THE JOURNAL OF PHILOSOPHY

THE IDENTITY OF INSTINCT AND HABIT

TWO years ago, in a brief paper entitled *Are There Any Instincts?*[1] I questioned the usefulness of the conception of instincts then prevalent in psychology, pointing out that while there are many reaction patterns which may legitimately be called instinctive—if by instinctive we mean *unlearned*—these reaction patterns are not combined in the so-called instincts in any exclusive way, but that the same reaction pattern occurs in several of the instincts, and that in some of the instincts practically all of the reaction patterns, learned and unlearned, which the animal possesses, function at one time or another. The "instincts," I concluded, are teleological groupings of activities, not psychological groupings. They are teleological, in that they are grouped with reference to the ends attained by the complex activity: not ends that the acting animal holds as conscious purposes, but ends that the classifier (the psychologist, biologist, philosopher, or whoever draws up the list of instincts), considers as attained by the activity in question. Lists of instincts, therefore, represent no fundamental psychological processes, but merely the convenience of the classifier: and any list which is convenient is as valid as any other list.

Since this heretical assault upon a widely held doctrine was delivered, a considerable number of writers have taken up the cudgel against instincts; and there seems to be danger that denunciation of instinct will become as fashionable and as uncritical as the acceptance of instincts has been hitherto. In particular, there seems to be a tendency to make no discrimination between instinctive activity, instinct, and instincts; but to assume that rejecting the last of these disposes of the others. It may well be that there is such a thing as instinct, namely, action determined solely by the environment (stimulation pattern), and the constitution of the animal; and that hence certain actions are properly called instinctive (without regard to what they accomplish in the world, of course); although the "instincts" are purely arbitrary groupings of activities. A recent author,[2] in fact, while rejecting the whole conception of instinct, reinstates instinctive activities under the changed names of inherited

[1] *Journal of Abnormal Psychology*, 1919, Vol. 14, pp. 307–311.

[2] Zing Yang Kuo, "Giving up Instincts in Psychology," this JOURNAL, 1921, Vol. 18, pp. 645–664.

"action-systems," defining these in precisely the same way in which we would ordinarily define instinctive tendencies.

It seems to me, therefore, necessary to state a little more carefully the objections to the old use of "instincts" in psychology, and also to add the consideration of certain objections to the antithesis usually assumed between instinct and habit, especially since these latter objections have not been raised by any of the recent authors to whom I refer. In my present view concerning this relation of instinct and habit, I am in part indebted to Dr. Ulrich,[3] although I must confess that the suggestion comes to me not from his important monograph, but from personal discussion with him of matters contained therein. I hope I may be pardoned if I repeat somewhat, and if I seem to introduce rather elementary illustrations, for I now feel that it is better to be a little tedious rather than run the risk of not making my point understood.

Psychologists have got into the habit of contrasting instinct and intelligence. From that they have gone on to the custom of speaking of specific instincts as if they were really separable entities;[4] a habit that was wished upon psychologists by a variety of men: philosophers, students of animal behavior, etc. The machinery of instincts so constructed has been seized on by several psychologists as ready-made apparatus for the construction of social psychology, and it behooves us to look rather carefully, if necessarily somewhat hastily, into the whole question of instincts, which, of course, we have to separate from instinct and from instinctive reaction.

We find in the animal, human and infra-human, tendencies to react in certain ways to certain stimuli: tendencies to make certain definite responses to certain definite features of the environment. For example: At a certain time the bell rings, whereupon the students in my room gather up their books and file out through the door. There is thus demonstrated a tendency to react in a particular way, however complex, to a particular stimulus pattern, which in this case happens to be a relatively simple one. The reaction, of course, was not in existence during the time immediately preceding the ringing of the bell. The students, however, did have the tendency. It may not have been in existence during the whole of the hour preceding the action, but certainly a number of minutes before the bell rang the tendency was there, ready to play its part in bringing about the action. A tendency, we may assume, is a certain arrangement in the nervous system; what Stout and others have

[3] "Integration of Movement in Learning in the Albino Rat," *Psychobiology*, 1920, Vol. 2, pp. 375–448, 455–500; *Journal of Comparative Psychology*, 1921, Vol. 1, pp. 1–96, 155–199, 221–286.

[4] The separation of intelligence into different intelligences has not been so easily accepted, although that separation has been proposed.

called a *disposition* of the nervous system: something which is definitely in existence, whether we describe it as a physical or chemical arrangement. It is because of this arrangement in the nervous system that the stimulus produces the response. That the "disposition" exists as an organic fact, is further proved by the fact that the reaction, in a case such as we have described, may be predicted.

As an illustration of the relation of "disposition," or "tendency," to response, we may consider an electric door bell and its operation. We press the button and the bell rings; because there was a mechanical arrangement of parts, an existing "disposition" of the sort that made the response (the transmission of the current and the ringing of the bell) possible when the stimulus (the pressing of the button) was applied. We mean by the "tendency" of an animal to react in a certain way to a certain stimulus, nothing more occult than the "tendency" of the bell to ring when the button is pressed.

In the human animal some of these tendencies are tendencies to perceptual reactions. If a certain canvas with certain paint smears on it is presented to one man, he perceives a genuine Tintoretto; while another person to whom is presented the same canvas perceives only a dreary daub. Why? The two men have different perceptual tendencies. There is a tendency on the part of one man to react to that stimulus in one way; and the other man has a tendency to react in another way.

Men have also divergent emotional tendencies. One man, while watching a certain scene on the stage, has an emotional result which we might describe as "interest, with mild pleasure"; another man reacts with a sad emotion, akin to grief. This difference can be stated as due to a difference in tendencies which have previously been established, so that the same stimulus pattern produces the responses in accordance with the different tendencies.

There are also thought-tendencies, as definite as the perceptual and emotional tendencies. If I mention Plymouth Rock to a group of people, some of them may think of chicken-runs, incubators, broilers, and market reports, while others may think of a stern and rock-bound coast, where the breaking waves dash high; depending on the particular tendency existing in the individual who is stimulated.

It is customary to divide or group these tendencies under two headings. We class them as instinctive, native, or inherited on the one hand, and habitual, learned, or acquired on the other. It is further customary to use the term "instinct" in a general way to indicate the existence in any animal of native reaction tendencies, "habit" in a corresponding way to indicate the existence of acquired

tendencies, and "intelligence" to mean the capacity to acquire or to modify reaction tendencies. With these methods of speech we have no quarrel, and may accept the terms as defined.

But there is a further custom of speaking of certain groups of instinctive tendencies as *instincts*. This usage is by no means a necessary consequence of those just described. We might use the term instinct in a general way, and speak in particular of instinctive reactions, and yet not speak of "an instinct" or "instincts" at all. The conception of instincts has been constructed, however, and various lists of "instincts" have been compiled, and we have come in a rather naïve way to speak of this or that "instinct" as if they were separable entities: either groups of reactions, or tendencies toward certain complex reactions. We speak of the "nesting instinct" of the bird, the "instinct of flight," the "parental instinct," etc., through various lists, these lists ranging all the way from those including but two instincts: the self-preservatory and the reproductive or race-preservative; through the list of four which Trotter considers adequate; to McDougall's list of twelve, and the list of Thorndike, which includes an indefinite number between ten and twenty.

The enumeration of such lists does not necessarily involve the distinction of "instincts" from simpler tendencies; but in the hands of all those who have constructed "fundamental" lists, such a distinction, as a matter of fact, is involved. Practically all of the compilers of lists have refused to admit to the group of "instincts" the simple reflex, such as the knee jerk, and many more complicated native reactions, such as the sucking of the child.

Criteria are therefore devised in order to distinguish instincts from other non-acquired tendencies. Most of the classifiers object to defining an instinct as a mere group of reflexes. McDougall assumes that consciousness is involved; that there are essential conscious elements in an instinct; and does not accept as an instinct an unconscious reaction, however definite. In a partial way, McDougall makes emotional accompaniment a criterion. In a certain number of the twelve instincts, at least, primary emotions are assumed to be involved. The instinct of flight involves the emotion of fear; repulsion involves disgust; curiosity, wonder; pugnacity, anger; self-abasement, subjection; self-assertion, elation; the parental instinct, tender emotion.

It is not my intention to discuss the various criteria of instinct; I merely mention these as illustrating the way in which psychologists have taken the term "instincts" in the plural, as contrasted with "instinctive reaction," which has not the same meaning.

From a purely physiological point of view, there are no instincts.

There are groups of re-activities (including not merely the end result, muscular and glandular activities, but the whole process in the nervous system and effectors), into which the minor groups enter in varying conditions. In the activities of flight, food-getting, and fighting, as they actually occur, when the "tendency" passes over into action, the same running movements may be present. To a large extent the running movement in getting away from some object which is inimical to the animal, the running movements in going after food of an elusive type, the running movements in pursuing an enemy, may be practically the same. In general, the same minor complex may enter now into this, now into that complex which is called an instinct, or the eventuation of an instinct. Some so-called instincts are at times entirely included in other instincts. For instance, flight, pugnacity, and food getting, taken just as groups of reactivities, may each occur as part of the parental instinct, since the parental instinct involves not merely the begetting of children, but also the procuring of food for them, and the fighting in their defense, and even the running away with a child at times, if danger is too threatening. We would have, then, from a physiological point of view, the instinct of flight and the instinct of pugnacity as parts of the total complex which would be called the parental instinct. It seems to me, moreover, that practically all of our activities enter at some time or other into the so-called reproductive instinct; and there are perhaps instances where the relation is reversed: where instinct *A* at one time includes instinct *B*, and at another time instinct *B* includes instinct *A*.

This inclusiveness and overlapping nature of the so-called instincts is not the point of greatest difficulty in classification. The really obstructive difficulty lies in the indefinite shading of one instinct into another. For example, between flight and pugnacity, even when it is not a question of their being included in some other instinct, the lines are by no means sharp; for between the two there is a continuous gradation of intermediate instincts.

If we attempt to distinguish instincts by the accompanying emotions, we again find difficulty. Fear, for example, appears sometimes as self-abasement, and shades by gradations into wonder. Moreover, fear is involved in a number of the instincts: the parental, the gregarious, and sometimes in the acquisitive. So also the subjective emotion and the emotion of elation are both found at times in the reproductive and parental instincts, and in the pugnacious instinct. I should say, moreover, that there are indefinite shadings of emotions between fear and disgust. The withdrawal from a situation may not involve fear alone or mere disgust, but may be marked by something between the two. So also there seems to be indefinite

shadings between fear and wonder, and fear and anger. It might be said that these shadings are mixtures; that we have in one case a mixture of fear and anger, and in another case a mixture of fear and wonder. That may be true. But we have no clearly marked "elemental" fear, or "pure" anger. Fear itself seems to be a complex which varies widely, and seems to contain elements which are also contained in anger and various other emotions, and at the present time it is safest to regard each of these so-called "primary" emotions as a complex of several elements, some of which are common to several emotions; and to regard the particular emotion present at any time as dependent upon the relative strength of the components. Excitement, for instance, is sometimes present in fear and usually in anger. Again, most of these "primary" emotions, such as fear or anger, are apparently of several kinds. The fear you have when you start running immediately does not *seem* of the same sort as the fear you have when you are struck immobile, and it is a question worth considering whether these *are* the same fear. To me, retrospection shows that they are not the same. Again, flight might occur without fear, or at least without the kind of fear which is usually associated with flight. I have found myself running, after a motor car has honked beside me on a street crossing, and yet have not found the inner content which I call fear. The emotion in such cases I should call "startle" or "being startled"; but not "fear."

I think the assumption that there is a very simple or constant thing to be called fear, another to be called anger, and so on, is certainly a dangerous one. If there are such constant things, the evidence so far does not demonstrate it. I can not see at the present time any great hope for the evolution of a list of "instincts" on the basis of the emotions.

If we should now attempt to distinguish reaction-tendencies on the basis of desire, or the purpose of the reacting animal (not the purpose of the classifiers), we might be on a better foundation. I think, myself, there is a distinct possibility there, and have been working on this line for several years. I should say, however, that in this we are getting away from the instinct basis altogether: that the classification of activities in accordance with their furtherance of desires is a very different problem from that in which McDougall, Watson, and Trotter have been interested. Such a system of distinction would depend, not on a primary classification of activity groups, but on a working out of the total activities of the organism now in this way, and now in that, from a strictly psychological point of view.

The actual basis of all the suggested lists of instincts is in the purposes of the *classifier,* not in the purpose of the *reacting animal.* All the unlearned activities of the animal which the classifier views

as contributing to the obtaining of food are considered by him as the "feeding instinct." All of those which, from the point of view of the classifier, culminate in the perpetuation of the species, are considered the "reproductive instinct." Any end or purpose which the classifier considers as important enough to set over against other ends is the point of departure for the erection of "an instinct." This is the teleological method, not the psychological. Now, from such a point of view, the classifier may erect as many instincts as will accomplish his own purposes. There is no reason for objecting to a "mathematical instinct," unless you do so on the ground of universality. There are results in the world which involve getting together the mathematical relationship of things, and the tendency to work towards these results is native to some people, if not to all. The "musical instinct," the "religious instinct," and many others, are also widely distributed. There certainly is such a thing as religion and activities which produce certain results which are designated as religious, and if we judge by history and by contemporary events, tendencies toward these sorts of activities are universal, and have a native basis in the constitution of human organisms. There is also a tendency in the human animal to construct a political system.

The popular writers who construct any instincts they please are quite in accord with the general system of instinct classification. In using the term "an instinct," you must conceive of a definite and describable type of result which may be attained by activities of various sorts, and assume that some of these activities are unlearned. Any system of classification which is adequate for your purpose is quite valid. A list of instincts is a good deal like a filing system: you may file all your documents under the letter of the alphabet with which the name of the writer begins, or you may file by subjects, or by dates. One system is useful in one business, another is more useful in another business. But an industry can not be founded on a filing system; neither can a system of social psychology be founded on a classification of instincts.

But after all, this difficulty with instincts is only a minor one. While I am glad to see that many persons interested in social psychology are beginning to doubt the usefulness of specific instincts as bases for work, I think there is a still graver difficulty in the whole question of instinct and instinctive reactions, a difficulty which rather seriously concerns not only the foundations of certain types of social psychology, but also some of our conceptions of education and eugenics.

We have been so far assuming that there is a fundamental difference, in human and other animals, between instinctive and acquired

reactions. Instinctive reactions we have tentatively supposed to be native tendencies to respond to stimuli in specific ways; and acquired reactions are supposed to be those tendencies which are derived from previous reactions and innate dispositions together. Starting with a tendency to some definite reaction to a definite stimulus, various other reactions modify this tendency and eventually an acquired tendency, distinguishably different from the original tendency, arises.

This conception of the two classes of reactions, easily accepted in the past, is indeed questionable. In the life of the higher animal, there are seldom, if ever, simple reactions to simple stimuli. The actual occurrences are complex stimulus patterns and complex reactions. We find some reactions, such as the knee-jerk, in which both stimulus and reaction are relatively simple; and some, such as the infant's sucking reaction, in which the stimulus seems simple, although the reaction is complex. But in the main, the actual adjustments of the organism to its environment are complex in both respects, and even the sucking reaction involves more of a stimulus pattern than we ordinarily suppose.

The stimulus pattern of an instinctive reaction, as such reactions are usually conceived, is assumed to be a purely spatial one: that is, not temporal also. In such a reaction, only the stimuli of the movement are effective. In the instinctive flying of a bird, assuming such to occur, the visual, tactual, kinesthetic, and perhaps auditory stimuli of the moment are assumed to be effective: the stimuli of preceding minutes, hours, and days, and the reactions evoked thereby, are assumed to be negligible. If these preceding stimuli and reactions do contribute to, or modify, the reaction, then the reaction is in so far not instinctive, but learned.

The characteristic of the learned reaction is just the temporal stimulus pattern. In playing billiards, for example, the movements of the arm and body are the results not of the stimulus of the moment alone, but are the results of a stimulus pattern which extends over a long period of time, perhaps months or years.

But both of these reactions, the acquired reaction and the so-called instinctive one, are equally "native." Suppose that a child is given a small piece of sandpaper at an early age, and that he puts that in his mouth, and subsequently cries. If the piece of sandpaper is given him repeatedly, the child eventually will react in a quite different way. Instead of grabbing the sandpaper, he will turn his head away and cry, and go through other reactions which express his intention of not putting the sandpaper in his mouth. The sandpaper in this case is the stimulus. At the first presentation, when the child put it in his mouth, we have a so-called instinctive reaction. At the last presentation, when he does not put it in his mouth but

does something else, we have a so-called acquired reaction. At the first time, it is assumed by current theory, the child's nervous system was so disposed that that particular stimulus, regardless of other stimuli preceding it, caused that particular reaction; and at the last time the child's nervous system was so constituted by heredity and the results of repeated stimulation, that the same stimulation at that particular time produced a different reaction. In the one case, there is a spatial, in the other a temporal, pattern. But after all, are not the two cases of the same kind, and equally "instinctive"? Is the difference between a temporal pattern and a spatial pattern great enough to be made so important, even if we should admit the possibility of a purely temporal pattern? The general statement of the reaction tendency in the two cases is much the same. If it is true that the child's organism, at the first trial, is so constituted by heredity that the first stimulation produces the first reaction (putting the sandpaper in the mouth), is it not equally true that his organism is at that moment so constituted that another stimulus (the repeated presentation of the sandpaper) will produce another reaction (avoiding the sandpaper)? Are not both reactions equally "instinctive"? Is not the reaction to a temporal stimulus pattern just as "native" as the reaction to a stimulus pattern merely spatially conditioned?

But, the reader may say, admitting that all reactions are equally "instinctive," both are not equally "acquired." Admitting that the second reaction is as fully native as the first, is there not something more in the second, or in the condition of the second? Even this we may very seriously deny. After all, there is no such thing as a "merely spatial" pattern: all patterns are temporal, and all reactions equally "acquired."

Let us consider such a complex reaction system as the nest-building of the robin. How could this occur without preceding stimuli and reactions involved in the processes of feeding, flying, *etc.*? Suppose the bird had not gone through this preceding reaction series. Can we assume that his nervous system could have developed to the point where the nest-building tendency would appear? We can not. In other words, a temporal pattern extending far back of the beginning of the nest-building, is involved; and our basis for distinguishing between this sort of reaction and habit completely disappears.

Suppose we consider some of the less complex reactions: the sucking reaction of the child, for example. Suppose the child had not first been stimulated by cold air, and pressure, in such a way as to produce the crying reaction: could the sucking reaction have been evoked later? Here, again, we can not get away from the effects

of preceding stimuli. In this case it might be said that we are not dealing with a specific effect on the nervous system in the way of modification, but with the preservation of the integrity of the nervous system. But this is a distinction without a valid difference. We are concerned with a condition in the nervous system which makes a certain specific reaction possible, and any other stimuli which are essential to putting the nervous system in such condition, or to maintaining it in such condition, through the reaction evoked, must be considered as a part of the stimulus pattern. Our knowledge of the nervous system does not permit us to go beyond this point.

At the present time, I can see no way of distinguishing usefully between instinct and habit. All reactions are definite responses to definite stimulus patterns, and the exact character of the response is determined in every case by the inherited constitution of the organism *and* the stimulus pattern. All reactions are instinctive: all are acquired. If we consider instinct, we find it to be the form and method of habit-formation: if we consider habit, we find it to be the way in which instinct exhibits itself. Practically, we use the term *instinctive reaction* to designate any reaction whose antecedents we do not care, at the time, to inquire into; by *acquired reaction,* on the other hand, we mean those reactions for whose antecedents we intend to give some account. But let us beware of founding a psychology, social, general, or individual, on such a distinction.

KNIGHT DUNLAP.

JOHNS HOPKINS UNIVERSITY.

MUST WE GIVE UP INSTINCTS IN PSYCHOLOGY?

IN a recent number of this JOURNAL (Vol. XVIII, No. 24) there appeared an article entitled "Giving Up Instincts in Psychology" by Mr. Zing Yang Kuo, of the University of California, in which the writer argued not only that instincts have been overworked as explanatory concepts in psychology, but that, as a matter of fact, there are no such things as instincts in human nature. This point of view and many of the considerations urged in behalf of it are interesting and stimulating. There are several points, however, at which the writer's argument appears to me to be loose and open to attack. For example, emphasis is laid on the fact that "there is no general agreement among the students of instincts as to the number and kinds of instincts." That there is such a lack of general agreement among students of instincts no one would deny. But this does not imply the non-existence of instincts; it merely reflects the lack of scientific accuracy and completeness in this field of investigation.

This lack of accuracy and completeness in enumerating and classifying the instincts is doubtless due in part to the relatively short period of time that instincts have been made the objects of scientific study. It may also be due to the specialized and partial points of view from which instincts have been considered. Students in this field have for the most part been concerned with instinctive tendencies as these are implicated in various social processes and institutions. It was inevitable, because of this circumstance, that confusion and differences should arise with reference to the number and kinds of instincts. But this confusion and these differences may be expected to gradually pass away as methods of study become more refined and the points of view from which investigations are made become more objective.

Again, the writer attempts to establish an analogy between the theory of instinct as implying *a priori* relation of the organism to the environment and the theory of innate ideas; and argues that one is as objectionable as the other. He says: "To assume any inborn tendency is to assume *a priori* relation between the organism and stimulating objects; for every behavior is an interaction between the organism and its surrounding objects. Such an assumption is no less objectionable than the theory of innate ideas. As a matter of fact both the theory of instinct and that of innate ideas are based on the same conception; namely, the conception of *a priori* relation of the organism to external objects" (p. 648). Now if we are warranted in speaking of an inborn tendency as implying *a priori* relation between organism and environment in an objectionable sense, a very large part of the structural and functional equipment of the organism must be regarded as involving the same implication. Relations between the organism and the environment in the way of behavior depend on and presuppose the skeletal system, vital organs, receptors and limbs quite as much as they depend on and presuppose tendencies to action, whether innate or acquired. There is a sense of course in which these relations may be said to be *a priori*, namely in the sense that the structures and functions which they bring into operation are fitted in advance of experience to interact with the environment in significant ways. But there is nothing mysterious or miraculous about *a priori* capacities in this sense. They represent the selective influence of the environment on the life stream from which the organism springs. And whatever there is of *a priori* character about an inborn tendency, *i.e.*, an instinct, is to be accounted for in the same way. Innate ideas, however, when they have been ascribed to the mind, have not been thought of as being *a priori* in this sense. Rather they have been referred to some transcendental source which has been set over against and contrasted

with the empirical and biological agencies conditioning our natures. Mr. Kuo's contention that "both the theory of instinct and that of innate ideas are based on the same conception; namely, that of *a priori* relation of the organism to external objects," overlooks the fact that the *a priori* character of the relation in the two cases is entirely different.

Furthermore, it would seem that while Mr. Kuo rejects the notion of an "inborn tendency" because it implies *a priori* relation of the organism to the environment, he neverthless is forced to presuppose this conception in accounting for the development of behavior. More specifically, it is admitted that "the human infant is endowed with a great number of units of reaction" (p. 658); and by "units of reaction" is meant the "elementary acts out of which various coordinated activities of later life are organized" (p. 658). Now these "units of reaction" or "elementary acts" must be presumed to involve innate neural tracts making possible just these responses and no others, however simple and undifferentiated in character they may be. We may call these responses "spontaneous" or "random"; but these are relative terms. They do not imply that we regard the responses in question as being accidental or unconditioned. We call such responses spontaneous or random because they do not seem to us to fall into any purposive system. And yet, they serve this purpose at least: they are the stuff out of which, as Mr. Kuo says, "the coördinated activities of later life are organized." At any rate, these "units of reaction" or "elementary acts" with which the individual is endowed at birth presuppose neural tracts which can only be described as "inborn tendencies" *i.e.*, tendencies to perform certain definite responses and no others; and as such they imply *a priori* relation between organism and environment of the same character which Mr. Kuo rejects in the case of instincts. Indeed, these "inborn tendencies" which become overt actions upon the presentation of the appropriate stimuli, are called, in another connection, "non-specific instincts" (p. 658). Whatever we may call them, they differ from instincts, as commonly understood, not in the fact that they are irrelevant to environmental conditions as represented by the stimuli which excite them, whereas instincts involve inborn tendencies which are relevant to the environment; but in the fact that they are not organized into systems serving specific biological ends.

But the argument at another point seems to me to imply the existence of instincts in this more specific and purposive sense. I have in mind the interpretation given Spaulding's experiment on the flight of birds. "That the birds could fly without previous education," says Mr. Kuo "was rather due to the maturity of reaction

system. . . . Given a mature reaction system and given an environmental demand, a definite reaction can fairly be predicted'' (p. 653). Now it is important to know just what the writer means to include within such a ''reaction system'' which is capable in advance of all education, when properly stimulated, of executing an intricate and significant action such as full-fledged flight. He speaks of the particular ''reaction system'' utilized in the flight of Spaulding's birds as including ''wings and other flying mechanisms'' (p. 653). What are these other ''flying mechanisms'' ? Do they not include nerve centers and nerve connections? And if so, must these not be thought of as forming and ripening in advance of experiences having to do with flight? To account for their tendency and ability, and the tendency and ability of other mechanisms involved, to execute adequate flying movements by reference to their maturity is beside the mark; the question is, did this state of maturity result from former efforts to fly? If not (as in the case of Spaulding's experiment), it must have developed out of conditions which were present in the organism at birth; in which case, I do not see that the notion of instinct can be excluded from a scientific interpretation of the facts.

It is true, of course, that any action, instinctive or acquired, is conditioned by the presentation of an appropriate stimulus. But the stimulus is not the cause of the action; there is no mechanical equivalence between the stimulus and the response that we are able to make out. It is only a cue for the execution of movements provided for in some ''reaction system.'' And given the stimulus and the ''reaction system'' we can predict the response to be expected only if we have in mind the purpose served by such mechanisms as are under consideration. We can be certain, for example, that birds kept in small boxes until their wings and other flying mechanisms have matured will fly when there is an environmental demand for this action, because we know in advance that wings and the mechanisms connected therewith are developed for flying. Without this advance information, turning the birds loose in the air with nothing to support them might, for all we could tell, result in any other action as well as in flight. And so it is in the case of any combination of stimulus and ''reaction system'' : our ability to predict a definite response always presupposes an insight into the functional character of the relationship involved. Mere knowledge of the stimulus as a brute fact and of the reaction mechanisms as so many structural entities is not sufficient. This means that the primary condition of significant activity, as well as of spontaneous or random movement, is internal rather than external. Whether we think of this condition in terms of McDougall's ''drives'' or ''springs to action'' or in terms of Mr. Kuo's ''reaction systems,'' the emphasis falls on the neural struc-

tures of the organism rather than on environmental factors. And however complex this inner tendency to action is, and however much of its complexity and significance it may owe, at any moment of its history, to the modifying influences of education or training, it presupposes a minimum core or foundation in the inherited structures of the organism without which it could not have had a beginning. The minimum core or foundation thus presupposed, in so far as it is inborn, and in so far as it makes possible significant interactions with the environment, is, it seems to me, on the basis of Mr. Kuo's own argument, deserving of the name "instinct."

J. R. GEIGER.

COLLEGE OF WILLIAM AND MARY.

THE MODIFICATION OF INSTINCT

IN an article on "The Modification of Instinct from the Standpoint of Social Psychology," published in the *Psychological Review*, volume 27, 1920, pp. 247–69, I took the position that instinct could be modified by habits formed prior to the instinct's appearance. As a partial support for this view, I cited the following observation made by C. O. Whitman upon pigeons:

> If a bird of one species is hatched and reared by a wholly different species, it is very apt when fully grown to prefer to mate with the species under which it has been reared. For example, a male passenger-pigeon that was reared with ring-doves and had remained with that species was ever ready, when fully grown, to mate with any ring-dove, but could never be induced to mate with one of his own species. I kept him away from ring-doves a whole season, in order to see what could be accomplished in the way of getting him mated finally with his own species, but he would never make any advances to the females, and whenever a ring-dove was seen or heard in the yard he was at once attentive.

Professor H. A. Carr, in editing the Whitman manuscript, also directed attention to the principle involved. Since the publication of my paper, this interpretation has been questioned first by Professor James Leuba in private correspondence and last by Mr. Zing Yang Kuo, in a most interesting paper upon "Giving up Instincts in Psychology," this JOURNAL, Volume 18, 1921, pp. 656–7. The criticism urged is that the behavior of the pigeons so modified was not an instinct but a habit. The passenger-pigeons' choices of mates from their own species are themselves, so it is said, the result of training and association and are not innate. Therefore, so the conclusion runs, we do not have the modification of an instinct, but merely the supplanting of one habit by another.

The proper understanding of much human and animal behavior depends to such an extent upon the principle here involved that I wish to suggest the answer to the problem as I see it:

For the principle concerned, it makes no difference whether the specific choice of mates by normally reared passenger-pigeons is chiefly inherited or acquired. The modification brought about is of the same general type as that found in the conditioned reflex, save for the temporal location of the modifying influences. Whitman's observation gives us a case which is scientifically described and convenient to use, but illustrations might be drawn as well from man's sex choices, from the field of sex education, and elsewhere. The sex instinct consists of those motor and glandular activities whose occurrence is due to an inherited synaptic connection giving certain sensory impulses ready access to the necessary final common paths. The instinct is certain behavior set up in muscles and glands. It is not a stimulus, nor yet an inherited synaptic connection, but is aroused by the former and controlled by the latter. Considered most accurately the *instinct* is not modified unless effector activities are changed. We are considering, however, as others have considered, the modification of the total inherited stimulus-response situation. The immediate problem is whether or not habitual associations formed prior to the appearance of the instinct have set certain stimuli as the ones which will be effective in arousing the instinct.

The detailed stimuli arousing sex behavior are unknown. In general, however, two classes of stimuli are involved: visceral sensory impulses corresponding to appetite, or desire, on the conscious side; and somatic sensory impulses aroused by the external object or by the symbol which represents it. It is probable that in many animals there is not a high degree of specificity on the somatic sensory side and that these factors are supplied to a high degree by the experience of the individual. However, the chance selection of somatic avenues seems to be weighted in favor of cutaneous, olfactory and possibly visual stimulation, *i.e.*, the somatic avenues do not seem to be equally open. On the visceral sensory side there seems to be an undoubted native connection between internal secretions and the appearance of sex responses in the somatic and visceral effectors (just as there is a connection between stomach contractions and feeding activities in normal animals). Exercise seems able only to vary this visceral sensory factor and so affect the intensity of the appetite. Whether or not this is actually the case, I do not know. (A diagram indicating the coexistence of the two types of sensory avenues is presented on page 253 of my paper above cited.)

What Whitman's observations show is that prior to the appearance of that typical form of response known as sex behavior the associations established within the lifetime of the pigeon have changed the stimuli which will later help in eliciting the response by

varying the synaptic connections on the somatic sensory side of the arc. The contrast between mating with members of the same species in the one case and with members of another species in the other case gives the essential fact. In neither instance need the stimulus be connected with the response through synapses set by heredity in order that the modification of instinctive behavior conform to the principle stated. If experiment should show that there is no somatic afferent connection set by heredity to arouse the mating response, the stimulus-response fact would of course be different from what I am inclined to assume, *viz.*, that along with the inherited motor grouping goes a more or less definite nervous organization favoring certain somatic stimuli. But such results, if secured, would only further confirm the fact that associations formed prior to the appearance of the instinct may modify it (either on the motor or on the sensory side) when it does appear. In the food-getting instinct in chicks one has an instinct similar to the sex instinct in that the sensory components of each include both somatic and visceral factors and in that the somatic stimuli are less definite than they later become. The chick pecks at first in response to visual stimulation from any small object or in response to an overpowering appetite in the absence of the proper external stimulus. With practise the somatic stimuli become confined largely to food objects. The significant difference between the food-getting and the mate-getting responses is that one appears shortly after birth and the other much later. The food-getting instinct therefore offers little opportunity for modification by experience either on the sensory or on the motor side prior to its appearance.

I do not regard it as the function of this paper to discuss the question of whether or not instincts do exist. The psychologists who are questioning the existence of inherited forms of response may do the science a service in forcing a more definite use of terms, but so far the prospects of attaining their avowed goal do not seem encouraging. To disprove the existence of instincts, one must either disprove the existence of reflexes or prove that there is a significant difference in kind between the behavior termed instinct and that termed reflex. Mr. Kuo discounts the idea of the existence of instincts partly because the behavior in question involves the coördination of simpler responses, and coördination he holds to be the result of habit. However, physiological work indicates that even the simplest reflexes are coördinated activities. The author also disputes the existence of delayed instincts, inherited forms of behavior appearing at varying intervals after birth. Various angles of this question have been long discussed, but I cite in opposition to Mr. Kuo's view only Lloyd Morgan's somewhat theoretical dis-

cussion of the moorhen's first dive and Yerkes's and Bloomfield's experimental observations on the behavior of kittens in killing mice. In order to disprove the existence of delayed instincts, it is not sufficient to indicate that elements of the response have been exercised before. It is necessary to account for the somewhat sudden grouping of the elements into a significantly new response under conditions where the influence of habit formation is experimentally controlled and negligible.

The opponents of instinct will also have a very considerable difficulty in handling such data, meager though it is, as that presented by Yerkes on the inheritance of savageness and wildness in rats and by Whitman on the hybridization of pigeon behavior.

There are other interesting—but I think seriously mistaken—points both in Mr. Kuo's paper and in other recent papers couched in a similar vein. My purpose, however, is merely to remove the misapprehension which has come to my notice with reference to an earlier proposition, *viz.*, that associations formed prior to the appearance of an instinct may modify the instinct when it appears.

WALTER S. HUNTER.

THE UNIVERSITY OF KANSAS.

BOOK REVIEWS

Mysticism, Freudianism and Scientific Psychology. KNIGHT DUNLAP. St. Louis: C. V. Mosby Company, 1920. Pp. 173.

There are two main points of interest in this stimulating essay: the pigeonholing of the Freudians with the mystics; and the reflex arc concept applied to perception and to the association of ideas.

Mysticism is illustrated by quotations from Plotinus and Dionysius, as well as from Maeterlinck and other modern mystics. It pretends to reach a "third kind of knowledge," additional to that gained through the senses and through inference.

This third and highest type of knowledge is to be had only in rare moments of absorption in an adored object, and is ineffable and incommunicable. The author concludes that this "knowledge" amounts to emotion, pure and simple, with the sex element strong in the emotional complex. But such emotional experience has no claims to scientific recognition as a source of knowledge.

"Pseudo-mysticism," exemplified by belief in spirit communication and telepathy, differs from the genuine article in pretending to employ logical inference in reaching its conclusions, but its logic is constantly vitiated by the use of ambiguous middle terms,

or "sliding terms," to use the author's expressive words. All mysticism is impatient of clearly defined terms, and of the whole painstaking procedure of science, and tries to solve the mysteries of life by uncritical short cuts.

Scientific procedure is strenuous and exacting. It requires constant touch with facts, parsimony in hypotheses, experimental testing of hypotheses, verification of results by independent observers, and careful definition of terms.

Freud is shelved with the mystics by the canny bookseller; Freud's popular appeal is to the love for the mystical. He and his followers shrink from the arduous road of science, and pretend to possess knowledge gained in some other way. His reasonings are vitiated by sliding terms. "Libido" is one such term; it means either the genuine sex motive, or something, oh! very much broader, just according to the momentary needs of the argument.

But the champion sliding term is *the Unconscious.* Sometimes the unconscious is simply a working hypothesis which proves to work well; and sometimes it is a fact from which there is no escape. Sometimes it is merely a "negative concept which can neither be described nor defined"; and sometimes "we are already familiar with a whole series of positive distinguishing features which differentiate the unconscious psychic material from the rest, the conscious and foreconscious." The unconscious is not merely physiological—far from that! It is a kind of unconscious consciousness, possessing the characteristics of consciousness when you need them for the purposes of your argument, but not when they would disturb the argument.

The Freudian confusion regarding the unconscious is in part chargeable against the long-standing ambiguity of the term, "consciousness," which has meant sometimes "awareness," and sometimes "objects of which one is directly aware."

> From this grand ambiguity trouble has arisen continually. The strife between " interactionism " and " parallelism ": the conception of thought and consciousness as stuff and things, and conversely, the conception of perceived objects as figments of " mind ": are only details of the results, of this confusion. . . . We eliminate at the start the metaphysical theories of epistemological dualism which have played so pernicious a role in the history of the Anglo-German psychology. Experience does not give us directly two worlds of objects physical and mental . . . but merely a world *of objects of which we are aware* (pp. 117, 120, 121).

Though the author does not speak of this conclusion of his as having any history, it seems rather reminiscent of things that have been said by Wundt, James, and other representatives of the objectionable "Anglo-German psychology."

> The ambiguity of the term " consciousness " is undoubtedly a factor con-

tributing to the Freudian confusion over the "unconscious." . . . But this verbal confusion although a great help to the theory of "unconscious mind" is not its vital source. . . . The believer in "unconscious mental processes" . . . might state his claim as follows: In addition to mental processes, *i.e.*, organic processes involving consciousness (awareness), there are other processes, which while they do not involve awareness, involve something which is more than mere physiological process: something resembling consciousness, but not conscious. This "unconscious mental" factor is therefore an x, an unknown, and can not be pointed out in any definite experience. Such an hypothesis might be made. One might also hypothesize a y factor, a z factor, and an infinity of other factors, all equally unknown, equally beyond experience" (pp. 125, 126).

Since, then, the hypothesis of an unconscious which is not purely physiological is "removed from any possibility of verification," it is entirely valueless to science. What its value or appeal may be to those who eagerly embrace it the author does not stop to inquire, but the answer is suggested in a highly critical review of this same book by the psychiatrist, John T. MacCurdy, M.D.[1] Dr. MacCurdy bases an argument for the unconscious, as against "physiology," on a law of creative synthesis, which can be seen in operation throughout the realm of natural science. Any synthetic combination shows properties that could not be predicted from the known properties of the synthesized elements—as, for example, the crystalline properties of common salt could not be inferred from the known properties of sodium and chlorine. Analytical chemistry tells you of the elements composing the compound, but, to know the compound itself, you must study it for itself, and not in terms of its elements. In the same way, biological processes may be analyzed into physical and chemical processes, but if for that reason we neglected to study life processes as such, we should never know much biology. In just the same way, mental processes are composed of physiological processes; but if we are satisfied to study them only from the physiological side, we shall never know much psychology. Psychology, the contention is, must stick to its own order of facts, and to its own concepts, and not take refuge in physiological interpretations. Therefore, it has need for the concept of unconscious mental processes. The value of the "unconscious," accordingly, is simply that it enables one to keep on using psychological terms, even when dealing with unconscious processes.

I shall not undertake any criticism of this interesting contention, except to remark that it serves better as a justification for the "co-conscious," in Morton Prince's sense, than for Freud's particular brand of unconscious. In double personality there is some evidence of a secondary synthesis of processes lying outside

[1] In an article entitled, "Psychiatry and Scientific Psychology," in *Mental Hygiene* for April, 1921, Vol. 5, pp. 239–265.

the main consciousness, and thus some reason for speaking of co-conscious mental processes; but in the case of dreams, lapses, queer impulses and fears, and unanalyzed motives, I do not know that anyone has seriously attempted to demonstrate the existence of any synthesis additional to that which occurs in the conscious process itself. The conscious process may show disturbance from some source outside itself; but, unless it could be shown that this outside source consisted of processes integrated into a total process of the mental order, we should, even according to Dr. MacCurdy's principle, regard it as physiological.

As against the unconscious in the sense of a storehouse of memories, Professor Dunlap puts the case with striking clearness:

> An idea is not a thing like a written document which, after being in the active files is taken out and stored in the transfer case. It is more like an *act* such as snapping the fingers or striking a blow. I may snap my fingers ten times in succession: but no one supposes that the snaps have an individual existence afterwards and are somewhere stored away as snaps which are no longer snapping. No more does scientific psychology conceive of "ideas" as something which can be stored away after they are through "ideating." In the one case as in the other, there is a physiological basis which is modified by the act in such a way that the act can be repeated at a future time (pp. 105–106).

Professor Dunlap, as the reviewer happens to know, has had his eye on the psychoanalysts for many years, and his remarks regarding them are by no means purely "academic." Some of these remarks deserve quotation.

> Reactions which later become a part of the general sex activity are found in the child, and therefore pointed out as evidence of sex activity. It is as if one should claim that the labored breathing produced by running to catch a street car is *sexual* because the same labored breathing may occur during certain stages of sex activity (p. 60).
>
> As is readily seen, anything that can be dreamed of has a ready sex interpretation. So that the telling of one's dreams to anyone versed in the gentle art of psychoanalysis is a matter in which your feelings of delicacy or prudence will dictate if you realize the possibilities (p. 71).
>
> This . . . characteristic is true in my opinion of all the cases in which the Freudian analysis "strikes oil." The situation which is discovered through analysis is one which is perfectly well known to the patient, but the patient is loath to confess it and does not realize its importance (pp. 79–80).
>
> The psychoanalyst like the philosophical mystic is essentially tender-minded, and can not endure the difficulties and disappointments of prosaic science. We are not surprised, therefore, to find over and above the essential logical fallacy on which the system is based, a characteristic naïveté in reasoning and a characteristic lack of orientation in facts (p. 93).
>
> It is probable that psychoanalysts do produce cures, or at least marked alleviation of the condition, of certain cases. In other cases the results are less desirable. The question of vital importance is whether the harm done by the general application of the method outweighs the good accomplished (p. 102).
>
> Sometimes, a complex is built up by prolonged psychoanalysis. The patient,

for example, is convinced that his neurosis is a result of the mother-complex; at first he is astonished at the psychoanalyst's discovery but by the copious use of symbolism, by the perversion of all the patient says and does, with that end in view, he is finally persuaded that the complex originated in him, and not in the psychoanalyst. By constant contemplation of the complex and its magic relationships, all the symptoms of the patient's troubles become closely associated with it. If now the psychoanalyst can exorcise the demon he has raised the patient may be cured. . . . In many cases, however, the demon refuses to be exorcised or if he complacently leaves, returns shortly with " seven worse than himself," and the latter state of the patient is worse than the first (pp. 102–103).

Psychology has been culpably negligent in regard to the study of the desires, and the one positive service which the Freudians have done is in emphasizing the incompetence of our information (and also of their own information) on this important subject (p. 159).

The psychologist who needs a little excitement should not fail to read Dunlap's book, and follow it with some of the reviews of it by psychoanalysts. Even MacCurdy, whose long review contains much serious criticism, starts off by attempting to obscure the issue with a dust-cloud of professional jealousies. He speaks of an "antagonism at present existing between psychiatrists and what we may term 'academic psychologists' . . . ." "Both the psychiatrists and the psychologists insist that they are the ones who should direct the study and treatment of those mental abnormalities which lead to social unrest, economic insufficiency, and crime, as well as to frank nervous and mental disease." And this in face of Dunlap's statement: "The development of a sound psychotherapeutics will certainly not be the work of the general psychologist. But when it is developed, it will be developed by psychopathologists of thorough training in general psychology" (p. 165). Later on in his review, referring to Dunlap's strictures on the practise of psychoanalysis, MacCurdy exclaims: "How can a college professor, presumably attending to his teaching and laboratory work, who has never had a medical education and has never seen a patient except with a layman's eyes—how can he have collected material justifying such sweeping conclusions?"

Observe, however, the reaction of an anonymous psychoanalyst in the *Journal of Nervous and Mental Diseases:*

The work in question is just such as one would expect of a blind rat caught in a maze—defeat, failure, going here and going there, misunderstanding, misquoting, misreading, a tissue of stupid distortions, clumsy misrepresentations, and silly evasions based upon a host of phobic prejudices. . . . The book, to us, is a ludicrous defense reaction. Psychoanalysis he particularly singles out as a special obstacle to science. Piffle! Nothing can obstruct science when it seeks to learn about reality, not even Dunlap's hobgoblin antics. The psychoanalytic hypotheses are rather object lessons to make-believe scientists who entrenched behind their academic chairs are screeching aloud their condemnation of many things they not only do not understand, but of the very essence of which they seem fundamentally

incapable ever of understanding by reason of their emotional limitations. . . . One would like further to characterize this unutterably stupid production, but already too much space has been wasted in noticing it at all.

Certainly Dunlap has produced a very stimulating essay!

His second main discussion, on the "Reaction Arc Hypothesis," is more apt to arouse the psychologist. The reaction arc concept is the old reflex arc concept, "transformed by scientific psychology" some time within the decade, 1910–1920. Apparently the transformation in question has been the author's own achievement, and has consisted in bringing perception and the association of ideas into line under the general conception of *complete sensorimotor reactions.*

The reaction arc hypothesis is opposed to the old "phrenological" conception of consciousness as "dependent on the specialized functions of certain groups of neurons set apart from the other sorts of neurons" (p. 132). The function of any one neuron is the same as that of any other neuron, *viz.*, "to be irritated or stimulated, and to irritate in turn another cell" (p. 130). "Mechanically, the function of the nervous system is the production of *responses*: that is, the action of effectors in certain ways, consequent upon specific action of receptors. . . . Certain definite responses, or *reactions* of the organism, are accompanied by, or involve, consciousness. . . . From these facts, the construction of the reaction-arc hypothesis is inevitable. Consciousness (awareness) is the result of, or the accompaniment of, or a part of (the phrasing is for the present immaterial)[2] certain reactions involving the activity of a complete arc from receptors to effectors" (pp. 133–134).

Seeing an object, for example, is a sensorimotor reaction, and requires the activity of a "complete arc."

In the case of vision, experiments on animals, and human clinical cases, make this point clear. Destroying the retinæ; cutting the optic nerves; cutting the optic tract behind the brain-stem; destroying the occipital lobes (of the cerebral hemispheres) to which the optic tracts lead; or cutting the connections between the occipital lobes and the rest of the cerebral hemispheres: produce the one and the same results—blindness—by interrupting the arcs from the visual receptors to the effector systems, and destroying the possibility of a visual reaction. There is no single system of efferent channels from the hemispheres which the visual reaction need follow: hence, to block completely the visual reactions by operation on the efferent side of the arc, all the efferent channels from the hemispheres would have to be cut. This would cut off the possibility of not only visual, but all reactions—and the patient would not survive (p. 135).

This last is certainly unfortunate, as it means that the "reaction arc hypothesis" can never be verified. We must, it would

[2] The reviewer would prefer to say, "an attribute of."

seem, relegate this hypothesis to the limbo of hypotheses "which are by their nature removed from any possibility of verification," and which are consequently valueless. We shall never know whether a person could see, with all the motor channels from his brain blocked off; and, since we can not know, why should we care?

Still, though the reaction arc hypothesis can never be positively verified, it might perhaps be *disproved* by some less drastic experiment. Some deduction from the hypothesis might conflict with facts. The author helps us on our way, for he deduces various consequences of his general hypothesis, and insists, moreover, that in thus applying it to various problems he does not need to introduce any accessory hypotheses.

He applies the hypothesis to the case of the conditioned reflex. Here we have two stimuli, originally giving two different reactions, but, through simultaneous activity, the two arcs "become connected in the cerebrum so that the current flowing in over the afferent part of the one, may now flow out over the efferent part of the other" (p. 143). Such interconnection of reaction arcs in the cerebrum explains "all habit formation, including both the development of perception, and the association of ideas" (pp. 143–144).

In the development of perception, the child may see and smell an orange simultaneously, and the arcs for the visual reaction and for the olfactory reaction, becoming connected in the cerebrum, enable the child later to make the olfactory response to the visual stimulus.

When he comes to the association of "ideas," the author makes use of the well-known scheme for the chaining together of a series of movements into a skilled performance, such as dancing. The reaction A, terminating in the contraction of certain muscles, excites the muscle spindles in those muscles, and thus initiates a second, proprioceptive reaction. Along with this proprioceptive reaction, there is simultaneously excited by an external stimulus the movement B; and the afferent part of the proprioceptive arc becomes connected in the cerebrum with the efferent part of the reaction B, quite after the pattern of the conditioned reflex. In this way, through the mediation of the proprioceptive arc, movement B becomes attached to movement A, and no longer requires its own original stimulus.

Now, since an "idea" is to be conceived as a sensorimotor reaction, it must give rise to a proprioceptive reaction in the way just explained, and this proprioceptive reaction (or the afferent half of it, extending from the muscle spindles to the brain), must be the intermediary between one idea and the next in an associated chain.

> If ideas are dependent on reactions, and if ideas are capable of association, it must be that the ideational reaction-arcs are of such a nature that the completion of one reaction may initiate another. Since reaction arcs terminate in muscles and in glands, it must be that in one of these tissues lie the necessary receptors of the thought-arcs. The receptors in glands are as yet conjectural, and the glandular response is not of such a nature that we could assume it to be the stimulus of reactions as prompt, as manifold, and as finely graded as thought-reactions apparently are. The striped muscles, however, are provided with a plentitude of receptors in the "muscle spindles," and the muscular responses are quick, finely graded, and of great complexity, competent to initiate reactions of an endless variety. The muscle-receptors are, therefore, in all probability, the beginnings of the thought-arcs (p. 147).

Here we have a very specific deduction from the general reaction arc hypothesis, and it appears that this deduction can be put to the test of facts. Since the muscle-spindle intermediary between two ideas is necessarily "finely graded, and of great complexity," and also highly specific, in order to give the specific sequence of "ideas," it certainly should follow that the amputation of an arm or a leg would break up many definite associations. Loss of the muscle sense, as in tabes, should break up associations of ideas, as it is known to interfere with skilled sequences of movement. Dr. MacCurdy, in the review already mentioned, brings forward still more striking evidence:

> If thought originates in muscle perceptions—and he evidently means this literally—then destruction of the muscles must abolish ideas. There are many nervous and muscular diseases in which muscle tissue disappears almost entirely. In these thinking is usually undisturbed. It is possible for an individual to live with his spinal cord divided in the neck so that there is no connection between the brain and any voluntary muscle except those of the head and neck and the diaphragm. Thinking is still possible and may be acute. In case it were urged that thoughts were initiated by the vocal, mouth and neck muscles, it is easy to point to diseases where these are atrophied or have their nerve supply cut off from the brain (p. 255).

This appears like a complete disproof of the reaction arc hypothesis as applied to the association of ideas. The only line of defense which our author has prepared against such an attack is briefly indicated in the following passage:

> The reactions are ultimately abbreviated or short-circuited. . . . The complete muscular reaction is necessary during the learning process, but is largely eliminated, in the interests of economy, after the series have been thoroughly mechanized (p. 153).

But against the adequacy of this defense it may be urged: (1) that, according to the hypothesis, at least some imperfectly mechanized associations must be lost by the amputation of a limb; (2) that amputation, *etc.*, should be a very serious handicap in the formation of new associations; and (3) that in admitting that,

after mechanization, a thought can function otherwise than as a complete arc, the author has notably receded from the boldness of his first position. At first he held that "ideas are dependent on reactions," *i.e.*, on complete sensorimotor reactions—since, according to the reaction arc hypothesis, all mental processes are so dependent. But now we learn that the complete reaction is largely eliminated, and still the ideational processes occur. Apparently, some deductions from his general hypothesis have seemed unacceptable to the author, and he has therefore introduced a qualification which amounts to abandoning the hypothesis in its original clean-cut form.

As a matter of fact, he did not succeed in holding fast to the hypothesis even while applying it to the conditioned reflex and to perception. He dealt not in complete arcs as his units, but in half-arcs. The afferent half of one arc became connected in the cerebrum with the efferent half of another arc. In the same way, though he began his interpretation of the association of ideas with the presupposition that he must deal in complete arcs, he had immediately to split up his proprioceptive arc, and let the afferent half of it function alone in mediating between one idea and another. Any theory which dealt consistently with complete arcs would have to seek the formation of new linkages *in the periphery!*

It is impossible to write a physiological psychology in terms of the "complete arc" as unit. Learning, whether motor, perceptive, or "ideational," requires a smaller unit. Facilitation and inhibition require a smaller unit. In the simplest cases of learning, such as the conditioned reflex, the half arc seems to make a workable unit. In reactions that involve a series of cerebral steps between the sensory stimulus and the motor response (as in the case of seeing a signal, knowing what it signifies, and responding with an appropriate skilled movement), the unit is smaller than a half arc, and amounts to a single one of the cerebral steps. In our author's fundamental statement, "that the function of any one neuron is the same as that of any other neuron, *viz.*, to be irritated or stimulated, and to irritate in turn another cell," there is certainly nothing that demands the "complete arc" as the unit. Rather, the unit should be the stimulation of one neuron by another. This is the single step in any psychoneural process; and there is no reason compelling us to adopt any larger unit as the ultimate unit of physiological psychology.

R. S. Woodworth.

Columbia University.

JOURNALS AND NEW BOOKS

THE BRITISH JOURNAL OF PSYCHOLOGY. June 1921. *The Instinctive Behavior and Enjoyment:* C. LLOYD MORGAN (pp. 1–30).–"If I were asked: 'What are you driving at in this and so much else that you have written on instinct?' the reply would be: 'My aim has been to find its place, as defined, in the evolutionary story of life and of consciousness.'" The manner in which the mind acts as a directive agency in determining the course of instinctive behavior and the way in which the mind acts as an impulsive agency, as the driving power behind the observed behavior must both be included; otherwise one has only a description in terms of observed facts which are merely generalizations that formulate the plan of the facts. *Is a Fatigue Test Possible?* B. MUSCIO (pp. 31–46).–An essential pre-condition of experimentation designed to obtain a fatigue test is the knowledge that different degrees of fatigue are present at certain times. It is justifiable to experiment with the object of finding a rapid and convenient fatigue test. It is recommended that the term *fatigue* be absolutely banished from precise scientific discussion, and that the problem to be investigated be defined as the determination of the effects of different kinds and amounts of work (activity) upon mental and physiological functions: that is, that the kind and amount of work be correlated *directly* with changes in psycho-physiological functions, and *not* (as at present) *indirectly* by means of *fatigue. A Preliminary Study of the Reproduction of Hand Movements:* C. RODRIGO LAVIN (pp. 47–52).–The problem was to determine the most favorable conditions for the learning and reproduction of hand movements. The conclusions indicate that (1) in the early stages of practise all of the subjects attended to the *form* of a movement rather than to its *extent;* (2) the points of movement most speedily and accurately learned were the beginning and end, and wherever sharp changes of direction occurred; (3) different forms of hand movement were very readily coalesced or "condensed." *Suggestibility with and without Prestige in Children:* F. AVELING and H. L. HARGREAVES (pp. 53–75).–The results show that general suggestibility is greatly modified by the specific conditions and elements of the whole situation, which vary in individual cases, according to experience of it and knowledge of it. There is no ascertained tendency for suggestibility to go with other "general" factors such as general intelligence, perseveration, oscillation, or motor dexterity. There is small correlation between it and common sense regarded as consisting largely of affective and conative elements; but none if common sense is considered as a purely cognative quality. *Recent Work in Experimental Æsthetics:* EDWARD BULLOUGH (pp. 76–99).–

No work of an experimental kind has, to the writer's knowledge, been carried out since 1914, and what is meant by "recent" are experiments undertaken between 1900 and 1914. It is the lack of experimental work during the last six years which prompted the writer to survey the earlier results. The writer emphasizes that until the conceptions with which philosophies of art are wont to operate are illuminated by actually and accurately observed experiences of many persons, instead of being vaguely apprehended and rashly generalized personal introspections of their authors, little will be done by interminable discussions of such topics. *Critical Notice:* G. UDNY YULE (pp. 100–107). A review of the revised and expanded edition of *The Essentials of Mental Measurement* by William Brown and Godfrey H. Thomson. *Publications Recently Received. British Psychological Society Membership List.*

Brierly, Susan S. An Introduction to Psychology. London: Methuen & Co. 1921. Pp. 152. 5 s.

Dopter, M. Les Maladies Infectieuses pendant la Guerre (Étude épidémiologique). Paris: Félix Alcan. 1921. Pp. 308. 9 fr.

Mandonnet, P. and Destrez, J. Bibliographie Thomiste. Le Saulchoir, Kain, Belgium. 1921. Pp. xxi + 116. 10 fr.

Moore, Jared Sparks. The Foundations of Psychology. Princeton University Press. 1921. Pp. 239. $3.

Myers, Caroline E. and Garry C. Measuring Minds: An Examiner's Manual to Accompany the Myers Mental Measure. New York and Chicago: Newson & Co. 1921. Pp. 55.

Sellars, Roy Wood. Evolutionary Naturalism. Chicago and London: The Open Court Publishing Co. 1922. Pp. xii + 343. $2.50.

Smallwood, William Martin. Man—the Animal. New York: The Macmillan Co. 1922. Pp. xiv + 223. $2.50.

de Unamuno, Miguel. The Tragic Sense of Life in Men and in Peoples. Translated by J. E. Crawford Flitch. (With an Introductory Essay by Salvador de Madariaga.) London: Macmillan & Co. 1921. Pp. xxxv + 332. 17 s.

NOTES AND NEWS

A copy of the second edition of *Moral Values and the Idea of God,* by W. R. Sorley has recently reached us. These Gifford Lectures of 1914 and 1915 were originally published in 1920, and were reviewed in our issue of November 18, 1920. The author prefixes this second

edition with the following brief preface: "In preparing this edition I have kept in view the criticisms of the book which have come into my hands, but I have not introduced new matters of controversy. The few errors which have been pointed out by others or discovered by myself have been corrected silently; and certain portions of the argument, where experience has shown that there was a possibility of misunderstanding, have been made clearer and more pointed in statement. The only addition which needs to be recorded is a short discussion on the relation of foreknowledge to freedom, a topic dealt with too dogmatically in the first edition."

The Southwestern Philosophical Association held its second annual meeting on December 28, 1921, as the guest of Occidental College. Dr. Bernard C. Ewer, of Pomona College, presented a paper on "A Dilemma in Ethics," and Dr. James Main Dixon, of the University of Southern California, one on "The Philosophy of Sympathy" as an expression of French and Scottish thinking. A discussion of Rivers's treatment of instinct and of the unconscious was offered by Dr. John Scott, of the University College of Cardiff, Wales, and this year Mills professor at the University of California. The following officers were elected for the coming year: *President,* Ralph Tyler Flewelling, University of Southern California; *Secretary-Treasurer,* Henry Nelson Wieman, Occidental College; *Member of the Executive Committee,* Bernard Capen Ewer, Pomona College. The next meeting of the Association will be at Easter time.

The Philosophy Section of the Oklahoma Educational Association met on Thursday, February 9, in Oklahoma City. Dr. Melvin Rigg of Oklahoma City College presented a paper on the History of the Logos Doctrine, which discussed St. John's conception of Christ as the Word of God, together with the antecedents of the Logos doctrine in Greek philosophy, and Hebrew theology.

The JOURNAL OF PHILOSOPHY has been unable for some time to fill orders for complete files because of the lack of two early numbers, Vol. II, No. 5, and Vol. III, No. 1 (March 2, 1905 and January 4, 1906). If any of our readers have copies of one or both of these issues which they would be willing to part with, the JOURNAL would be very glad to buy them at the rate of 50 cents a copy.

THE JOURNAL OF PHILOSOPHY

There is no similar journal in the field of scientific philosophy. It is issued fortnightly and permits the quick publication of short contributions, prompt reviews, and timely discussions. The contents of the last five issues are as follows:

Volume XVIII. No. 25. December 8, 1921

The Future of Philosophy as a University Study. WILLIAM ADAMS BROWN.

The Value of Theories. RALPH M. EATON.

Reviews and Abstracts of Literature. Journals and New Books. Notes and News.

Volume XVIII. No. 26. December 22, 1921

On My Friendly Critics. GEORGE SANTAYANA.

A Discussion of "Mind Discerned." MAURICE PICARD.

Reviews and Abstracts of Literature. Journals and New Books. Notes and News.

Volume XIX. No. 1. January 5, 1922

Pragmatism and the New Materialism. ARTHUR O. LOVEJOY.

A Partial Analysis of Faith. LUCINDA PEARL BOGGS.

Book Reviews. Journals and New Books. Notes and News.

Volume XIX. No. 2. January 19, 1922

An Analysis of Reflective Thought. JOHN DEWEY.

The Nervous System, Psychological Fact or Fiction? J. R. KANTOR.

Book Reviews. Journals and New Books. Notes and News.

Volume XIX. No. 3. February 2, 1922

On the Method of Metaphysics. CHARLES H. TOLL.

Critical Realism. B. H. BODE.

Book Reviews. Journals and New Books. Notes and News.

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Psychological Review—Princeton, N. J.; Psychological Review Company.
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Subscription $5. 720 pages annually. Psychological literature.
Monthly. Founded 1904. Edited by Shepherd I. Franz.

Psychological Monographs—Princeton, N. J.; Psychological Review Company
Subscription $5.50 per vol. 500 pp. Founded 1895. Ed. by James R. Angell.
Published without fixed dates, each issue one or more researches.

Psychological Index—Princeton, N. J.; Psychological Review Company.
Subscription $1.50. 200 pp. Founded 1895. Edited by Madison Bentley.
An annual bibliography of psychological literature.

Journal of Philosophy—New York; Sub-Station 84.
Subscription $4. 728 pages per volume. Founded 1904.
Bi-weekly. Edited by F. J. E. Woodbridge and Wendell T. Bush.

Archives of Psychology—Sub-Station 84, N. Y.; Archives of Psychology.
Subscription $5. 600 pp. ann. Founded 1906. Ed. by R. S. Woodworth.
Published without fixed dates, each number a single experimental study.

Journal of Abnormal Psychology—Boston; Richard G. Badger.
Subscription $5. 480 pages annually. Edited by Morton Prince.
Bi-monthly. Founded 1906. Entire field of abnormal psychology.

Psychological Clinic—Philadelphia; Psychological Clinic Press.
Subscription $2.50. 288 pages. Ed. by Lightner Witmer. Founded 1907
Without fixed dates (9 numbers). Orthogenics, psychology, hygiene.

Training School Bulletin—Vineland, N. J.; The Training School.
Subscription $1. 160 pages ann. Ed. by E. R. Johnstone. Founded 1904.
Monthly (10 numbers). Psychology and training of defectives.

Journal of Educational Psychology—Baltimore; Warwick & York.
Subscription $4. 540 pages annually. Founded 1910.
Monthly (9 numbers). Managing Editor, J. Carleton Bell.
(Educational Psychology Monographs. Edited by Guy M. Whipple.
Published separately at varying prices. Same publishers.)

The Behavior Monographs—Cambridge, Mass.; Emerson Hall.
Subscription $5. 450 pages per volume. Edited by John B. Watson.
Published without fixed dates, each number a single research.

Psychoanalytic Review—Washington, D. C.; 3617 10th St., N. W.
Subscription $6. 500 pages annually. Psychoanalysis.
Quarterly. Founded 1913. Ed. by W. A. White and S. E. Jelliffe.

Journal of Experimental Psychology—Princeton, N. J.
Psychological Review Company. 480 pages annually. Experimental.
Subscription $4.25. Founded 1916. Bi-monthly. Ed. by John B. Watson

Journal of Applied Psychology—Worcester, Mass.; Florence Chandler.
Subscription $4. 400 pages annually. Founded 1917.
Quarterly. Edited by G. Stanley Hall and L. R. Geissler.

Psychobiology—Baltimore: Williams and Wilkins Company.
Subscription $5. 500–600 pages per volume.
Bi-monthly. Edited by Knight Dunlap. Founded 1917.